and bad; but in the former it is as likely to be altogether absent, while the health of trees is irretrievably ruined.

There is no branch of gardening in which inexperienced or fanciful persons do more harm than in pruning. They seem to forget that fruit trees are grown for the sake of their fruit and not as objects of decoration, and that three ends, and three only, are to be gained by the operation; that is to say, increase of quantity, improvement of quality, and better ripening. Nothing but skilful pruning will effect these purposes; unskilful, in which is to be included unnecessary pruning, has a directly opposite tendency. In short, the golden rule in this case is NOT TO PRUNE AT ALL IF IT CAN BE AVOIDED. Pruning however is unavoidable; but it should be had recourse to as little as possible. As to overpruning, it is we repeat far worse than no pruning at all. One thing is certain, that the more Apple trees are pruned the less they bear; and the same may be said of Pear trees.

The author of one of our best practical works, having described how an Apple tree should be managed for the first three or four years, remarks, "after this nothing more will be necessary than to look them (the trees) over from time to time, *cutting out carefully any superabundant branches* that may appear, particularly those which have a tendency to injure the proper figure of the head, or are likely to become stronger than the rest: these latter, if suffered to remain, will injure any description of tree, whether it be a standard, an espalier, or whether it be trained against a wall" (*Guide to the Orchard*, p. 118). This is the best advice that can be given to those who have the management of Apple trees in an orchard. It is like the worthy Mr. GLASSE's instructions to "let them alone." But our Suffolk correspondent's trees are in a state of ruin. They seem to be like the Devonshire trees, which Mr. BELFIELD describes with "heads tangled and matted together so as to set both sun and air at defiance; live wood struggling for existence amongst the dead, and all hoary with Moss and premature old age." (*Journal of Bath and West of England Society, III.* 195.)

With such trees the pruning knife and saw must be used unsparingly; and if that is what our Suffolk correspondent's gardener means by "scientifically" we agree with him. Not that there is much science in the operation. The *first* thing to do is to cut down to the quick every dead branch, limb, or spur; they can do no good, and are mischievous on account of the interruption they offer to sun and air, which are as necessary to the tree as to the gardener. Until this has been done live wood should remain untouched. *Secondly*, as soon as the dead wood is gone, and the gardener can see *distinctly* what he has to work upon he should prune out every shoot that whips or crosses or rubs against another, so as to leave plenty of room between the shoots; a foot is not too much. In doing this the weakest shoots should be removed. *Thirdly*, all the thinning having been done, the end of each branch should be stopped by removing more or less of it according to its strength. *Fourthly*, after the stopping, all loose bark and Moss should be scraped off the branches and main stem with the blade of an old hoe or some such blunt edge, and the scrapings should be burnt. In this way alone can insects and their eggs be destroyed with certainty. Such scraping can do no harm; and in addition to the removal of insects it enables the tree to breathe more freely; a very important matter, for the living bark is as much a portion of an Apple tree's lungs as the leaves are. This done skill can go no further, and it is only necessary afterwards to leave the tree to its own vital powers; watching however how the new shoots grow, and cutting out from time to time all such as in any way whip, chafe, or cross each other.

In these remarks the state of the soil is not noticed. If however there is any doubt about its being thoroughly drained that also must be carefully looked to, for no Apple trees can retain their health in waterlogged ground. Neither can they prosper when soil is exhausted of all its nutritive matter. When that is the case weak manure, such as plenty of decayed leaf-mould, burnt weeds, or any similar material should be employed. Strong ammoniacal manure is to be avoided.

THE notion of making VINE BORDERS with carrion has always encountered resolute opposition in these columns, whatever favour it may have found elsewhere. Dead pigs, dead horses, rotten sheep, offal and garbage of all sorts, whatever the "practical" authority with which they were puffed off, were alike repugnant to common sense, and to rational gardening. Doubtless a few crops of great coarse Grapes might be had by their use, if the Vines did not die; but in the long run destruction to the Vines was inevitable. Nevertheless, misled by the authority of a name, some unwise gardeners allowed themselves to be led by their noses, and paid the penalty of their egregious folly.

It was at Raby that garbage-fed Vines grew for a while in all the glory of rankness and corpulency. It was to Raby that the admirers of carrion-grown Grapes used to point. Where are the glories of Raby now? Let one of our good correspondents tell what he has lately seen there.

"Having had frequent opportunities," says "R. T.," "of seeing the Raby Vines for the last eight years, and as so much has been said about them, and the wonders they were to perform, a few remarks on them may not be uninteresting since they are now condemned. Having by chance heard that the charge of the gardens had passed into other hands, and that the destruction of the Vine borders was in progress, I was anxious to see the state of the Vine roots.

"An interval of three years had elapsed since I last saw the Vines, and what a falling off was there; berries, bunches, and wood, had become 'small by degrees and beautifully less.' In fact there was not in that splendid range of four Vineries (180 feet by 20 feet) one dozen good bunches fit to send to a nobleman's table. I found the original border had contracted by decomposition and other causes to less than 12 inches in depth, and had become a compact, stinking, adhesive mass like putty; the roots appeared to have left this carrion bed some years back, as all the roots in it were dead, and the few roots that remained alive were in the soil above, made by top dressings to cover the roots, annually drawn to the top by the large quantities of hot dung laid on the borders. The wood of the Vines is wretched in the extreme. A sort of hit or miss system (or rather course) of pruning, or get a bunch where you can, has been followed, till all became one mass of confusion and failure; letting alone the mortification to the noble owner, who has spared no expense to secure for his table an ample supply of fruit.

"New borders are now in course of preparation, those for the early houses being chambered and heated with four rows of hot-water pipes; the soil used, as far as I could see, turfy loam, lime rubbish, charcoal, and rotten dung; some of the old Vines will be replanted, young ones being planted with them."

We are happy to learn that his Grace is not likely again to suffer from practical crotchets.

WITH reference to the experiments on RINGING THE VINE, last week communicated by Mr. RIVERS, that gentleman has sent us another bunch of Black Hamburgh Grapes which has evidently suffered injury; and he states that several more are in the same condition, while bunches on branches not ringed are all perfect and good. In the instance now before us we incline to the belief that the ringing has been too severe; either too deep or too broad, or both. For we find the wood below the ring very nearly dead, which would of course render it impossible for the Grapes to reach maturity. Undoubtedly the degree of ringing that may be allowed is a very important point for determination. Our own opinion is that the ring ought not to be wider than is necessary to prevent the return of the sap by the bark, nor deeper than the bark itself, care being taken not to injure the alburnum.

ON THE AGENCY OF BEES

IN THE FERTILISATION OF PAPILIONACEOUS FLOWERS, AND ON THE CROSSING OF KIDNEY BEANS.

LAST year you published a brief notice by me on this subject. I therein stated that bees always alight on the left wing-petal of the Scarlet Kidney Bean, and in doing so depress it; and this acts on the tubular and spiral keel-petal, which causes the pistil to protrude; on the pistil there is a brush of hairs, and by the repeated movement of the keel-petal the hairs brush the pollen beyond the anthers on to the stigmatic surface. This complex contrivance led me to suppose that bees were necessary to the fertilisation of the flower; accordingly I enclosed some few flowers in bottles and under gauze, and those which were not in any way moved did not set a single pod, whereas some of those which I moved in imitation of the bees produced fine pods. But I then stated that the experiment was tried on much too small a scale to be trusted. I have this year covered up between 3 and 4 feet in length of a row of Kidney Beans, just before the flowers opened, in a tall bag of very thin net; nothing in the appearance of the plants would lead me to suppose that this was in any way injurious to their fertilisation; and I think this conclusion may be trusted, for some of the flowers which I moved in the same way as the bees do produced pods quite as fine as could be found in the uncovered rows.

The result was that the covered up plants had produced by August 13th only 35 pods, and in no one case two pods on the same stalk; whereas the adjoining uncovered rows were crowded with clusters of pods. There were many flowers still on the plants when uncovered, and it was curious to see in a few days afterwards, as soon as the bees had access to them, what a number of pods hanging in clusters of three and four together were produced. On August 17th I again put the net on a later crop. The covered plants now produced 97 pods, borne on 74 stalks, showing that the same stalk often produced more than one pod. This time I kept an equal length of uncovered Beans ungathered, and on this length there were 292 pods or exactly thrice as many as on the covered plants. Taking this number as the standard of comparison for the first experiment (which, however, is hardly fair, as my gardener thinks the second crop was more productive than the first) more than eight times as many pods were produced on the uncovered as on the covered rows. The Kidney Bean is largely frequented by the thrips, and as I have with some other plants actually seen a thrips which was dusted with pollen leave several granules on the stigma, it is quite possible that the fertilisation of the covered-up flowers might have been thus aided.

In the common Bean there is no such obvious relation between the structure of the flower and the visits of bees: yet when these insects alight on the wing-petals they cause the rectangularly bent pistil and the pollen to protrude through the slit in the keel-petal. I was led to try the effect of covering them up, from a statement in the *Gardeners' Chronicle* made several years ago, viz., that when bees bite holes through the calyx of the flower in order to get more easily at the nectar, the crop is injured. This was attributed by the writer to injury of the ovarium, which I am sure is incorrect. But I thought that it was possible that the fertilisation would be less perfect, as soon as bees ceased to alight on the wing-petals. I accordingly covered up 17 plants, just before the flowers opened, moving a few flowers to ascertain that very fine pods, including the full average number of Beans, could be, and were, produced on the plants under the net. These 17 plants produced 36 pods; but no less than eight of them, though well formed, did not include a single Bean. The 36 pods together contained only 40 Beans, and, if the empty pods be excluded, each produced on an average less than one and a half Beans; on the other hand 17 uncovered plants in an adjoining row which were visited by the bees produced 45 pods, all including Beans, 135 in number, or on an average exactly three Beans to each pod. So that the uncovered Beans were nearly thrice as fertile as the covered.

In an old number of the *Gardeners' Chronicle* an extract is given from a New Zealand newspaper in which much surprise is expressed that the introduced Clover never seeded freely until the hive-bee was introduced. This statement may be erroneous; at least, as I shall immediately show, it does not apply to the Canterbury settlement. But I was induced by it to cover up under the same open sort of net about a yard square of the common White Clover, growing thickly in turf; and then gathered an equal number of heads from the covered and from some uncovered plants which were growing all round, and which I had seen daily visited by my bees. I collected the seed into a small parcel, and as far as I could estimate, the uncovered plants produced just ten times as much seed as the covered. Speaking loosely, the covered heads might have been said to have produced no seed.

Lathyrus grandiflorus is very rarely visited by bees in this country; and from experiments which I have tried during the two last summers, and from experiments recorded in Loudon's Magazine, I am convinced that moving the flowers favours their fertilisation, even when the young pod falls off, as very often happens almost immediately. Sir W. Macarthur, who did not know of my experiments, told me that he had found that in New South Wales the introduced Erythrina did not set its pods well without the flowers were moved. From the statement in regard to the Clover in New Zealand, I wrote to Mr. Swale, of Christchurch in New Zealand, and asked him whether Leguminous plants seeded there freely before the hive bee was introduced; and he in the most obliging manner has sent me a list of 24 plants of this order, which seeded abundantly before bees were introduced. And as he states that there is no indigenous bee (perhaps this statement applies to bees resembling hive or humble bees, for some other genera are known to inhabit New Zealand), the fact that these plants seeded freely at first appears quite fatal to my doctrine. But Mr. Swale adds that he believes that three species of a wasp-like insect performed the part of bees, before the introduction of the latter; unfortunately he does not expressly state that he has seen them sucking the flower. He further adds a remarkable statement, that there are two or three kinds of grasshoppers which frequent flowers, and he says he has repeatedly watched them "release the stamens from the keel-petal." So that, extraordinary as the fact is, it would appear that grasshoppers, though having a mouth so differently constructed, in New Zealand have to a certain extent the habits of bees. Mr. Swale further adds, that the garden varieties of the Lupine seed less freely than any other Leguminous plant in New Zealand, and he says, "I have for amusement during the summer released the stamens with a

pin, and a pod of seed has always rewarded me for my trouble, and the adjoining flowers not so served have all proved blind." The case of the Lupine in New Zealand not seeding freely now that bees have been introduced, may be accounted for by the fact, if I dare trust my memory, that in England this plant is visited by humble bees and not by hive bees.

These several facts, and the foregoing experiments, seem to me rather curious; for who, seeing that papilionaceous flowers are hermaphrodite, have an abundant supply of pollen, which is mature before the flower opens, and that the flower itself is so neatly closed, would have imagined that insects played so important a part in their fertilisation? I can hardly doubt that in England, during a season when bees were very scanty, if in any one district large crops of seed-Clover were planted, the crop would partly fail from the flowers not being sufficiently moved.

The foregoing little experiments, however, were not tried in relation to the agency of insects in fertilising a plant with its own pollen. Andrew Knight many years ago propounded the doctrine that no plant self-fertilises itself for a perpetuity of generations. After pretty close investigation of the subject, I am strongly inclined to believe that this is a law of nature throughout the vegetable and animal kingdoms. I am well aware that there are several cases of difficulty.

The Leguminosæ with papilionaceous flowers have been advanced by Pallas and others as a case in which crossing could never naturally take place. But any plant habitually visited by insects in such a manner that their hairy bodies, to which pollen so readily adheres, come into contact with the stigma, could hardly fail occasionally to receive the pollen from another individual of the same species. In all Leguminosæ bees do brush over the stigma. And the possibility of crossing would be very strong in the case of any plant, if the agency of insects were necessary for its self-fertilisation; for it would show that it was habitually visited by them.

From these considerations I was led to believe that papilionaceous plants must be occasionally crossed. Nevertheless I must confess that from such evidence as I have been able to acquire, crossing between varieties growing close together does not take place nearly so freely as I should have expected. As far as I am aware only three or four cases of such crosses are on record. It is not by any means, I believe, a common practice with seed raisers to keep the crops of their Leguminous plants separate. Hence I was led last year in my short communication to the *Gardeners' Chronicle* to ask whether any of your readers had any experience on the natural crossing of Beans, Peas, &c. Mr. Coe, of Knowle, near Fareham, Hants, in the most obliging manner sent me some specimens, and an account that last summer he had planted four rows of the Negro Dwarf Kidney Bean, between some rows of the white and brown dwarfs, and likewise near some Scarlet Runners. The dwarfs he had saved for seed. The plants themselves he believes presented nothing remarkable in foliage, height, flowers, &c.; and he feels sure that their pods were all alike; but the Beans themselves presented an extraordinary mixture, as I can testify from the sample sent me, of all shades between light brown and black, and a few mottled with white; not one-fifth of the Beans, perhaps much less, were pure Negroes. Some few of the Beans also in the rows of the white Haricot were affected, but none of the brown dwarfs.

Hence, then, we apparently have the extraordinary fact described by Wiegmann in the case of several Leguminous plants, experimentised on most carefully by Gärtner in the case of the Pea, and described a few years ago by Mr. Berkeley in the *Gardeners' Chronicle*, of the pollen of one variety having affected not only the embryo but the tunics of the seed borne by the pure mother. I have said that apparently we have here a fact of this nature; for I must state that Mr. Coe sent me a dozen of the pure Negro Beans which produced in 1857 the extraordinary mixture. I sowed them this year, and though quite like each other, the dozen produced plants differing in colour of flower, &c., and Beans of various tints; so that these Beans, though not affected in their outer tunics, seem to have been the product of a cross in the previous year of 1856.

This year I sowed the extraordinary mixture raised by Mr. Coe in 1857 from the four rows of the Negro Bean, which he believes to have been quite pure; and the produce is the most extraordinarily heterogeneous mixture which can be conceived; each plant differing from the others in tallness, foliage, colour, and size of flower, time of ripening and flowering, size, shape, and colour of pods, and Beans of every conceivable tint from black to pale brown, some dark purple and some slightly mottled, and of various sizes and shapes. My gardener remarked, as did Mr. Coe with respect to some of his plants, that some of the seedlings seemed to have been crossed by the Scarlet Runner; one of my plants trailed on the ground for a length of 4 feet, its flowers were white and its pods were very long, flat, and broad; the Beans were pinkish purple, and twice as large as those of the Negro; there were also in two cases brown and purple Beans in the same pod. These facts certainly seem to indicate a cross from the Scarlet Runner; but as the latter is generally esteemed a distinct species, I feel very doubtful on this head; and we should remember that it is well established that Mongrels frequently, or even generally, are much more vigorous than either of their parents.

Mr. Coe tried the experiment more philosophically, and separated his heterogeneous Negro Beans into 12 lots, according to their tints, and keeping a few of each as a sample, he sowed them and he has now harvested them separately. He has kindly sent me samples of all. The variation is now much greater than it was in the parent lot of 1857. Beans of new colours have appeared, such as pure white, bright purple, yellow, and many are much mottled. Not one of the 12 lots has transmitted its own tint to all the Beans produced by it; nevertheless, the dark Beans have clearly produced a greater number of dark, and the light coloured Beans a greater number of light colour. The mottling seems to have been strongly inherited, but always increased. To give one case of the greatest variability, a dirty brown Bean, nearly intermediate in tint between the darkest and lightest, produced a sample, which I have been enabled to divide into no less than a dozen different tints, viz., pure white, black, purple, yellow, and eight other tints between brown, slate, yellow, purple, or black. It has been stated that a few of the white Haricots in the rows adjoining the Negroes were in 1857 slightly affected; Mr. Coe sowed some which were of a very pale brown or cream-coloured; and he has sent me a pod produced this autumn, which pod includes two Beans of the above tint and one of a pale dirty purplish-brown.

Now it may be asked are we justified in attributing this extraordinary amount of variation to crossing, whether or not the crossing was all confined to the year 1857; or may not the case be one of simple variation? I think we must reject the latter alternative. For in the first place the Negro Bean is an old variety and is reputed to be very true; in the second place, I do not believe any case is on record of a vast number of plants of the same variety all sporting at the very same period. On the other hand, the Negroes having been planted between rows of white and brown Beans, together with the facts which I have given on the importance of insect agency in the fertilisation of the Kidney Bean, showing, as may be daily seen, how incessantly the flowers are visited by bees, strongly favours the theory of crossing. Moreover the extraordinary increase in variability in the second generation strikingly confirms this conclusion, for extreme variability in the offspring from mongrels has been observed by all who have attended to this subject.

As seed-raisers do not usually take any precautions in separating their crops of Leguminous plants, it may be asked, how are we to account for the extraordinary amount of crossing in Mr. Coe's plants in 1857, when almost every plant in the four rows of the Negro seems to have been affected? I may here add that in an old paper in the Journal of the Bath Agricultural Society there is an almost exactly parallel account of the crossing of several varieties of the common Bean throughout a whole field. Insect agency is always at work; but the movement of the corolla will generally tend merely to push the flower's own pollen, which is mature as soon as the flower is open, on to the stigmatic surface; and even if pollen is brought by the bees from another flower, the chances are in favour of pollen from the same variety being brought, where a large stock is cultivated.

I can explain Mr. Coe's case, and that in the Bath Journal, only on one hypothesis, viz., that from some cause the Negro Beans did not at Knowle, in 1857, produce good pollen, or they matured it later than usual. This has been shown by Gärtner sometimes to occur, and would explain with the aid of insect agency the whole case. Believing, as I do, that it is a law of nature that every organic being should occasionally be crossed with a distinct individual of the same species, and seeing that the structure of papilionaceous flowers causes the plant's own pollen to be pushed on to its own stigma, I am inclined to speculate a little further. It is, I think, well ascertained that very close interbreeding tends to produce sterility, at least amongst animals. Moreover, in plants it has been ascertained that the male organs fail in fertility more readily than the female organs, both from hybridity and from other causes, and further that they resume their fertility slower, when a hybrid is crossed in successive generations with either pure parent, than do the female organs. May we not then suppose in the case of Leguminous plants, after a long course of self-fertilisation, that the pollen begins to fail, and then, and not till then, the plants are eagerly ready to receive pollen from some other variety? Can this be connected with the apparently short duration and constant succession of new varieties amongst our Peas, and as is stated to be the case on the Continent with Kidney Beans?

These speculations may be valueless, but I venture earnestly to request any of your correspondents who may have noticed any analogous facts on sudden and large variation in their seed-crops of any Leguminous plants (including Sweet Peas), or any facts on such plants having kept true for many consecutive generations, when grown near each other, to have the kindness to take the trouble to communicate them to the *Gardeners' Chronicle*, or to the following address. *C. Darwin, Downe, Bromley, Kent.*

Home Correspondence.

Pampas Grass.—There are two specimens growing here on turf, one having produced 35, the other 27 spikes of inflorescence from 10 to 12 feet in height. This plant only requires to become more widely known to be fully appreciated. Either for planting in mixed shrubberies to remove the dull monotony of their line, or for grouping or otherwise on turf it seems admirably adapted. As far as practicable it should be planted in sheltered situations, the heavy winds and rain of autumn being liable to damage its silvery spikes. *J. C., Primley Hill, Paington.*——There are now in bloom at Lover's Knowle, Kingsbridge, the property of J. Luscombe, Esq., of Combe Royal, Devon, two specimens of this plant bearing respectively 50 and 37 spikes of flowers, their stems being from 10 to 12 feet high. The situation in which they are growing is high, sunny, and dry, but the plants received liquid manure from time to time and copious supplies of water during the period of summer. The flowers were fully developed at Michaelmas, a month earlier than usual. In the American Garden at Combe Royal the male Pampas Grass has been in bloom, but it is very inferior to the female, being of a faint purplish tint. *A Devonian.*——We have a fine plant of this now in flower; it was planted out in 1856 when it had one spike, last year it had eight, and this year it has 27, the highest being about 12 feet high. *E. P. N., Glamorganshire.*

Sorghum saccharatum, which I should think will be soon familiarly known by farmers under its English name of the Chinese Sugar-cane, is very different from Sorghum vulgare. Your correspondent Mr. Eliot will find, if he consults Paxton's Botanical Dictionary, that the former was introduced (I suppose merely as a botanical plant) in 1759,* the latter in 1796. S. saccharatum tillers rather extensively, and throws up several canes if planted thinly. I have, Oct. 6, had a portion of my crop, which was sown May 20, cut and weighed; it gives at the rate of 36 tons 14 cwt. per acre. This was grown on a light sandy loam, and is about 6 feet high; the soil is not rich, but was stirred deeply with the fork. The second growth, from that which was cut the first week in August, is about 18 inches high; specimens of both are sent for your inspection, as are also some roots with their canes of Sorghum "Imphey," the African Sorghum: this is much more leafy than S. saccharatum, and consequently not so heavy. This the first cutting gives at the rate of 36 tons 5 cwt. per acre, and has been grown in an old sand-pit stirred 1 foot deep with the fork; it was transplanted about the middle of June, no manure given to the soil, but a top-dressing of the (not very powerful) sewage manure was strewn over the rows as soon as the plants had rooted. As far as I can at present judge the S. saccharatum is the most profitable to grow; with the stems of this I send some pieces of its canes partially dried, being a portion of my crop that I intend drying and having cut into chaff. You will observe how full of sugar these dried canes are; my horses have now had it, freshly cut, every day in lieu of hay for eight weeks, and have thriven well, being in better condition than usual at this season, after having had hay for the same period. It will, I fear, scarcely be believed that 20 square rods, or half a rood, lasted three horses six weeks. *Holcus.*

Hive-Bees in New Zealand.—The hive bee was introduced into Wellington in 1842 and into Canterbury in 1852. In Christchurch, in the latter province, an old hive standing in a warm sheltered situation has this summer cast off six swarms during the short time of two months. English bee keepers would open their eyes with astonishment if they were out here to see the produce of a single hive. I have had the pleasure several times of partaking of the fruit of their industry, and most delicious it is. Bee keeping here is different to what it is in England. The perpetual succession of flowers, the fine warm summer, and mildness of the winter all tend to a great increase of the bees. Our management of them is very simple. We furnish them with small boxes 18 inches or 2 feet in length and a foot or 18 inches in depth, with a small aperture on the sunny side for ingress and egress. Inside the box we fix small rails across for them to commence building their combs. I have seen very severe conflicts between them and the native wasps. When a wasp approaches the hive the bees give no quarter. They soon slay their enemy and down with him. *Extract of a letter to Mr. Darwin from Mr. Swale of Christchurch, New Zealand, dated July* 13, 1858.

Water Lilies.—My master has a lake. The lake is so infested with these plants as to be hardly passable in a boat. They send their great fangs into the soft deep mud, and we can neither pull them out nor dig them out. It would be extremely obliging of any of my brother gardeners who have overcome a difficulty of the same kind if they would communicate their experience for the information of a *Novice in Aquatics.*

Roses in Derbyshire.—At page 114 it is stated that Roses die off about Buxton in a year or two after they are planted. For tender kinds, such as Bourbons, Noisettes, and varieties of similar habit, no kind of protection is better than partially enveloping their heads in dry Fern, secured against the effects of winds and snow by a stout stake. This covering put on before Christmas, and towards the end of March gradually removed, answers well here. But to avoid the necessity of covering at all plant hardy Hybrid Perpetuals, than which no Roses can be more beautiful, and instead of tall Standards select them not more than 3 feet in height. From many years' experience I find that the latter succeed much better than when worked on tall stems, particularly in exposed localities. I may add that if removed during the first or second season after planting, on Mr. Rivers's principle, the chances are 10

* The recent introduction of this kind of Sorghum to Europe was by the French Consul at Shanghai, who sent it to France a few years since.